Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-177-RC2, 2019

Geoscientific Model Development Discussions

Interactive comment on "The Canadian Earth System Model version 5 (CanESM5.0.3)" *by* Neil C. Swart et al.

Anonymous Referee #2

Received and published: 20 August 2019

Review of Swart et al. 2019: The Canadian Earth System Model version 5 (CanESM5).

General Comments:

This paper gives a clear and detailed description of the new Canadian Earth System model (CanESM5). The paper nicely describes the main developments relative to earlier Canadian models (e.g. CanESM2) and also provides a good overview of the primary model performance, sensitivities and associated biases, touching on (i) pre-industrial coupled climate stability, (ii) performance over the recent historical past and (iii) first-order sensitivity to increasing CO2 concentrations. It will be a useful reference to users of the model and to climate analysts using CanESM5 CMIP6 results. I recommend the paper be published, conditional on one relatively "major" comment and a number of more "minor" comments being addressed. These are detailed below.

Major Comment:

The paper assesses the performance of CanESM5 over the latter part of the CMIP6 historical simulation period, using a number of standard diagnostics where CanESM5 is compared to observations. A number of model biases/shortcomings are identified, but the likely/probable cause of these biases is never discussed. While I recognise it is not straightforward to fully identify the underlying cause of coupled model errors, it would be a useful addition to the paper if, at least for the most important biases (e.g. excessive surface temperature trends post-1980, precipitation and cloud biases, excess heat uptake in the tropical ocean thermocline etc) some discussion of the cause of these biases was provided. This does not have to be exhaustive and can point to a deeper analysis and discussion to come elsewhere, but some pointers to the main causes of key biases would give the paper a bit more "meat". This comment also applies to the significant change (increase) in TCR and ECS in CanESM5 relative to CanESM2, a somewhat greater discussion on what is considered the leading cause(s) for this increase seems appropriate.

Minor comments:

Minor comments are listed with reference to the line numbers in the paper. Where I think some additional explanation would improve the paper I outline this within each point.

1. L50: "CanESM5 represents a major update to CanESM2": The reader is left wondering what happened to CanESM3 and 4 ?? Is there some reason naming jumped from "2" to "5"?

2. L86-L87: "the emission of mineral dust and DMS was improved. . .". It would be useful to know the degree to which dust, DMS and other natural (aerosol) emissions are fully prognostic in the model, allowing potential future feedbacks to be simulated, versus these using observation-based (and therefore time-invariant) prescribed input fields.

3. L101-L102: “The introduction of dynamic wetlands and their methane emissions is a new biogeochemical process. . .”. Is methane a prognostic atmospheric variable in CanESM5? e.g. how do the wetland methane emissions influence the model radiation? There is no mention of interactive chemistry in CanESM5, I therefore assume methane is not a prognostic variable and that there are no internal (model) feedbacks between wetland methane emissions and climate. How methane (and other non-CO2 radiatively important gases e.g. ozone) are treated in CanESM5 needs to be more clearly explained.

4. L121: Why does CLASS use 4 PFTs but CTEM use 9? And is vegetation type/amount dynamically predicted in CanESM5 or (externally) prescribed? Please state what is done for the latter and explain the rationale for the former.

5. L304-306: It sound as though the primary tuning for the coupled pre-industrial model simulation was global mean surface temperature. Is this correct? It would seem to me that (a zero) global mean net top of atmosphere radiation would be a more appropriate target. The resulting net TOA balance looks to fairly close to zero (0.11Wm-2, figure 5a). Was the TOA net radiation budget also used as a tuning target?

6. L318-319: “The final adjustment was to the carbon uptake over land. . .. . .achieved via the parameter which controls the strength of the CO2 fertilization effect”. Does this indicate some structural problems with the parameterization of CO2 fertilization? I ask because in L813-815 there is mention of; “significantly larger uptake of CO2 by the land biosphere in CanESM5 relative to CanESM2 in the 1pctCO2 experiment”. Is this change in land uptake influenced by the tuning applied to the historical period CO2 fertilization term? Allied to this does CanESM5 include any representation of nutrient limitation of the land CO2 fertilization term?

7. L320: “Critically, no tuning was undertaken on the climate system response to forcing”. This is not strictly true as the authors say that they tuned the model to improve aspects of the historical simulation, which does include perturbed CO2 forcing.

8. Figure 4: This figure appears on page 15 but is not discussed until much later. In itself that is okay, but then it might be worth adding a statement where the figure is first shown, saying why this figure is sufficiently important to be shown at the start of this section and that further discussion of the figure results appear in sections xx or yy.

9. Figure 5 (caption). Some of the figures are explicitly listed as being “global” or “global mean” while others are not. Those not classified as “global” or “global mean”, if they are not, for what spatial region are they representative? If they are all global quantities then make this clear in the figure caption.

10. Figure 6 (caption): “Variables are labelled according to the names in the CMIP6 data request“ As not all readers will be CMIP6 experts, it could be useful to list what this shorthand list means in real word descriptions, possibly in a table in an appendix.

11. L520-521: Some of the precipitation biases, while large in percentage terms, are actually very small in absolute terms (e.g. the subtropical East Pacific). Would it be better to plot absolute precipitation biases? as done for temperature and SLP.

12. For Figure 7 (and possibly 8). I suggest showing DJF and JJA seasonal means rather than annual means. This will help focus on biases and reduce the risk of error cancellation within the annual mean, e.g. winter vs summer surface temperature biases, better focus on summer vs winter monsoon precipitation biases etc.

13. Figure 8 and L530: It would be useful to plot TOA solar and longwave radiation fields and biases (e.g. vs CERES EBAF) alongside the cloud plots, given that the number 1 consequence of cloud errors are in their impact on radiation.

14. L589-591: The wording in these lines does not make sense.

15. Figure 13: (caption) “The shading presents the corresponding inter-quartile range”: What range are we referring to here? For the model results is the range across ensemble members? What is the range with respect to the observations?

16. Figure 14: What occurrence of surface temperature and precipitation is plotted?

Daily mean values, monthly means ? Please state what is plotted.

17. L615-616: “Negative annual mean SSS biases occur under the region associated with excessive March sea-ice. . .” This seems counter to what I would expect. Generally with sea-ice formation there is brine rejection into the surface water, hence with excessive sea ice I would expect excessive brine rejection and a positive bias in SSS.

18. L676-678: Please provide references to PIOMAS and GIOMAS.

19. Figure 20: The excessive model sea-ice in the Labrador sea, does this have an impact on deep water formation and potentially help explain the weak ($\sim$12Sv) AMOC in CanESM5?

20. Figure 20 needs a clearer figure caption and explanation. It is not clear whether the plot shows ocean to atmosphere or atmosphere to ocean C flux. I am guessing positive values in the North Atlantic indicate ingassing (e.g. atmosphere to ocean flux of C). The implied direction of the flux with respect to positive and negative values needs to be made clearer. Also figure b looks like the difference in flux CanESM5 minus Landschutzer, the figure caption suggests it is an absolute value of Landschutzer.

21. L773-774: Why use this definition of the Northern Annular Mode? Is this a standard circulation measure? Would it not be better to plot the NAO for the Northern hemisphere?

22. Figure 24 (caption) : “The colour scale is arbitrary” What does this mean? Presumably the scales are the same across the 4 figures ?

23. L813-816: The TCR and ECS both increase (by $\sim$16% and 50%) in CanESM5 relative to CanESM, yet the TCRE decreases by $\sim$16%. This is unusual. It is explained as being due to a “significantly larger uptake of CO2 by the land biosphere in CanESM5 relative to CanESM2”. Given the stated importance of TCRE for policymakers, are the authors confident that the increased CO2 uptake is due to realistic and understood reasons?

24. L821-825: “ECS IS 5.7K”: Actually, in figure 4 the global mean surface temperature does not look to have sufficiently stabilized by year 140 to derive an ECS value (e.g. it increase by $\sim$0.75C over years 90 to 150). Have the authors tried plotting the so-called Gregory linear regression of nett TOA radiation versus surface temperature to see whether there is a single or multiple linear relationships between these two, or whether a non-linear behaviour is evident?

25. Figure 26c: It is difficult to see any differences. This might be better plotted as a model to observation difference or ratio.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-177, 2019.